

in quoting my name from a former volume of *NATURE* in 1872, as having then announced a cold wave of climate temperature being due to commence in 1878·8; or, as he is kind enough to say, the very day on which the severe winter of the years 1878 and 1879 did begin.

I must decline, at all events, the full honour, for these two reasons: *firstly*, the quoted datum was intended by me to signify the middle, not the beginning, of that cold wave; and as that lasted thirteen months, my date was between six and seven months too soon. *Secondly*, when I published again on the subject in the *Edinburgh Astr. Observations* of 1877, I indicated not 1878·8, but 1877·8 as the probable date of the then coming, now past, wave of cold, or erred much more than in 1872.

I have recently been looking into the reason of that latter calamitous failure, and find it clearly enough now on the plates of projections in that volume in this fact—that the then expected minimum of solar spots was inserted for 1877·5, or nearly two years sooner than sun observations have since then shown it to be. Rectifying, therefore, now that quantity, and in the then absence of our rock thermometers (which had first indicated the remarkable cycles of temperature in striking connection with the whole period, though not the details, of the sun-spots) using merely air-temperatures and re-drawing the curves, the middle epoch of the late cold wave comes out 1879·1, and of the next hot wave 1880·8.

Now these hot waves, I have always maintained, are more important, more regular, and more directly of solar origin, than the cold waves which are rather due to some indirect earthly effects of watery vapour and its transformations. It may be worth while, therefore, when we are now, as I believe, on the threshold of one of the heat-waves, to mention shortly the data on which the expectation is founded.

These are mainly the actual observations of no less than five occasions of such maxima of temperature occurring in a peculiar kind of dependence on successive sun-spot minima; or rather on the beginning, in each case, of the forces of a new spot cycle. The last such heat-wave was in 1868·8, or 1·8 year after the then last sun-spot minimum. Next before that in 1857·9, or 1·9 years after its previous sun-spot minimum. Before that, in 1846·4, or 2·4 years after the same solar test. Before that again, but then depending only on old atmospheric temperature observations, as our rock thermometers were not then existent, in 1834·5, or 1 year after; and still further back, in 1826·5, or also about 1 year after the then last sun-spot minimum.

These intervals from one to the other of the hot waves are by no means arithmetically equal, being 10·9, 11·5, 11·9, and 8·0. This last (really the earliest of the series) is a frightful inequality, but is borne out, *first*, by the sun-spot period of that cycle as given by M. Schwabe, having been also anomalously short; and *second*, by this remarkable testimony, which I have only just become acquainted with, in a pamphlet of most independent character by Sir Robert Christison, Bart., M.D., on tree-growth, read before the Botanical Society in Edinburgh:—

"The wonderful season of 1826, when warmth and sunshine, commencing with March, ended only with September, and when the summer was continuously such as to change in some respects the habits of the people."

The year 1826 was therefore a crucial case, not only for a maximum of temperature and sunshine in Scotland, but for its keeping such remarkable pace with the then anomalously shortened period of sun-spots. While the presently coming case of 1880 will equally prove, by what I have detailed above, that no certain success in weather predictions for several years beforehand can be hoped for, unless the dates of sun-spot minima can be also announced by authority beforehand to a very much smaller quantity than two years of error; and that no *mean* duration of the sun-spot cycle comes close enough to the fact of the large variations between one cycle and another. We must therefore each cycle of sun-spots fixed by its own dates alone, and not smoothed away and improved out of creation by being made apparently conformable to others.

I have not yet seen, by those able men who believe they have traced sun-spots to planetary influences of position on the sun, any attempt, from the planetary places in almanacs, to compute the dates of all the solar minima of spots, say from 1825 to 1900. But something of that kind appears now to be necessary for the next steps of the science of the future.

PIAZZI SMYTH

15, Royal Terrace, Edinburgh, January 9

### Cranial Measurements

In the notice of my catalogue of crania which you have been good enough to insert in *NATURE*, vol. xxi. p. 222, your reviewer has given me credit for originality of method, to which I have no wish to lay claim.

1. In reference to the mode of taking the horizontal circumference, it is said that I pass "the tape line, not over the prominence of the glabella, as is customary with craniologists, but above it, around the supra-orbital line." The fact is, that the method which I have adopted, so far from being a deviation from what is customary, is that recommended in the valuable "Instructions Craniologiques," drawn up by Broca and published by the French Anthropological Society, and which is used, certainly by the large majority, if not by all the craniologists with whose writings I am acquainted.

2. With regard to the more important measurement of the antero-posterior diameter of the cranium, more important on account of its influence on some of the most characteristic indices, there are, unfortunately, still considerable differences of method, and it was only after very full consideration of the subject that I decided not to follow the French instructions, but to adopt the plan used by Rolleston in "British Barrows," by Barnard Davis in his "Thesaurus Craniorum," and by the majority of German anthropologists. So fully was I convinced of the expediency of the latter, that after having already measured the whole of the crania in the collection, and calculated the indices by the method which included the prominence of the glabella in the cranial length, I took the trouble to remeasure them, with the results given in the catalogue. The object being to obtain, as near as may be, an idea of the form of the brain-case, it appears desirable to exclude all extraneous projections which have no relation to this form. The impossibility of eliminating every source of fallacy, such as those occasioned by the varying thickness of bone or of diploe, is no argument against endeavouring to reduce them, as far as we can, to a minimum. The projection caused by the development of the frontal sinuses should certainly not be omitted in a complete description of a skull, but it no more affects the form of the cranium proper, than the prominence of the nasal bones or of the maxilla, which, important and instructive as they are from other points of view, are usually ignored in giving what is called the maximum length of the skull, although if the term is to be taken in its literal sense, they have as much claim to be included as the glabella or supra-orbital ridges.

Many other arguments might be adduced and authorities given for the usage I have adopted, but I will bear in mind your request for brevity.

W. H. FLOWER

Royal College of Surgeons, January 11

### "Why the Air at the Equator is not Hotter in January than in July"—Freezing of the Neva

IN *NATURE*, vol. xxi. p. 129, Mr. Croll gives his reasons why the equator is not much warmer in January than in July, notwithstanding the greater nearness of the sun at the former season. To state the case briefly, he, having recalled the fact that the whole earth is colder in January than in July, because in the former the cold winter of the northern (or principally land) hemisphere coincides with the mild winter of the southern (or principally water) hemisphere, he continues: "Consequently the air which the equatorial regions receive from the trades must have a higher temperature in July than in January. The northern is the dominant hemisphere; it pours in hot air in July and cold air in January, and this effect is not counterbalanced by the air from the opposite hemisphere. The mean temperature of the air passing into the equatorial regions ought therefore to be much higher in July than in January, and this it no doubt would be were it not for the counteracting effects of eccentricity." And further: "There is another case which must also tend to lower the January and raise the July temperature of the equator: the northern trades pass farther south, and consequently cool the equatorial regions more during the former than the latter season."

I maintain that there is no such influence of the northern trades on the temperature of the equator, because they scarcely anywhere reach it, and then because the lower latitudes of the northern hemisphere are not colder in January than those of the southern hemisphere in July. In the Atlantic the northern trades do not reach the equator at all in January, but only in February, March, and April, and this but in the western part of the ocean.

The same may be said of the Pacific in its eastern part, where alone the trades are regular. In the Western Pacific, as well as in the Western Indian Ocean, I admit that air from the northern hemisphere reaches to the equator and somewhat beyond in January, but not that this tends to give the equator a lower temperature in this month than in July. According to Dove, the mean temperature of  $10^{\circ}$  N. in January is  $77^{\circ}2$ ; of  $10^{\circ}$  S. in July,  $76^{\circ}1$ .

*So far as the temperature of the equator is concerned, the southern is the dominant hemisphere, and the equator is certainly cooled by winds coming from the south. If the equator is not everywhere warmer in January than in July, this is caused by the rainy season, which on the equator, and even a few degrees north of it, generally coincides with the southern summer. Where the rains are not very heavy, as, for example, on the Isle of St. Thomas, West Africa, we have: January,  $78^{\circ}3$ ; July,  $75^{\circ}7$ ; at Padang, Sumatra, where the rains are exceedingly heavy all the year, there is scarcely any difference at all between the months. Somewhat to the south of the equator, where to the difference in the nearness of the sun is added a much greater height above the horizon in January, we have—*

Jan. July. Rainy Season.

Amboina, Molucca Islands, {  $80^{\circ}9$   $77^{\circ}4$  May to August.  
 $4^{\circ}$  S. ... ... ... }  $78^{\circ}3$  July,  $75^{\circ}7$  ; at Padang, Sumatra, where the rains are exceedingly heavy all the year, there is scarcely any difference at all between the months. Somewhat to the south of the equator, where to the difference in the nearness of the sun is added a much greater height above the horizon in January, we have—

so, by the first-rate observations of Batavia, it is established that, so far as  $6^{\circ}$  S., January is  $1^{\circ}1$  colder than July, because the former is very rainy, while the latter has little rain. Even to  $9^{\circ}$  lat. N., July is colder than January, if the former has much more rain, so for example—

Island of Fernando Po, W. {  $79^{\circ}9$   $76^{\circ}5$  March to November.  
Africa,  $4^{\circ}$  N. ... ... ... }  $78^{\circ}3$  July,  $75^{\circ}7$  April to August.  
Gondokoro, Upper Nile,  $5^{\circ}$  {  $81^{\circ}3$   $75^{\circ}7$  April to August.  
N. ... ... ... }  $81^{\circ}3$   $75^{\circ}7$  April to August.  
Freetown, W. Africa,  $8^{\circ}3$  N.  $80^{\circ}4$   $77^{\circ}0$  June to October.

Thus, in the lowest latitudes of the northern hemisphere, we find differences amounting to  $5^{\circ}6$ , while in the southern greater differences than  $1^{\circ}1$  are not known, which may, to a certain degree, be ascribed to the nearness of the sun in January.

I think I have proved that, as to what we call the temperature of the air (really that of the lowest stratum), it is, on the equator and a few degrees north and south from it, far more influenced by the yearly distribution of clouds and rain than by the different amount of heat received from the sun. The result would be different if we knew the temperature of the whole stratum of air. The heating of the upper surface of the clouds by the sun, and especially the heat liberated in the condensation of water must give to the higher strata a superior temperature than that they had in the dry season; in other words, the decrease of temperature with elevation is much slower during the rains than in the dry season, as was shown for India by Mr. Blanford. This is true for other regions, and where the sky is cloudy and rains abundant in the greater part of the year, the temperature of the whole air may yet be higher than in drier climates, where the soil and the lower stratum of air are hotter.

I do not agree with Mr. Croll in what he states at the end of his letter as to the effect of winds in cooling the equatorial regions and rendering them habitable, as they would be too hot for man without the cool air brought from the temperate regions. I think Mr. Croll has enormously over-stated the effects of winds on the temperature of the equator. The extent of the tropical zone is so great, its temperature so very near to that of the equator, the winds which blow across it so gentle, that I consider the effect of the winds from the temperate regions in directly cooling the temperature of the equator to be nearly imperceptible. The following is a good illustration:—Nowhere is the winter temperature so low near the tropics as in Southern China, for example, in January, Canton,  $55^{\circ}6$ , Victoria, Hong-Kong,  $59^{\circ}2$ . Yet Saigon, in Cochin China, but  $11^{\circ}$  to the south of Hong-Kong, and subjected to the full force of the north-east monsoon from the China seas, has a January temperature above  $77^{\circ}$ . Clearly the thermal effect even of the cold winter monsoon is scarcely perceptible farther south.

I consider water to be the only direct cause of the mildness and uniformity of equatorial temperatures, and this in three ways—(1) by the great heat-capacity of water; (2) by the clouds

which interpose a screen between the sun and the surface of the earth; (3) by the evaporation of rain water by the soil and plants.

The first cause is especially powerful on the ocean, while the two latter act especially on land, even very far from the sea. If it was not for the clouds and evaporation, how could we explain, for example, the absence of great heat (hottest month,  $78^{\circ}6$ ) at Iquitos, on the Amazons,  $4^{\circ}$  S., and more than 1,000 miles from the Atlantic, where the winds are generally weak?

As to the winds, I admit of their effect in this case; but (1) in causing ocean-currents, and thus removing the heated water from the equator; (2) in spreading the cold air from over the cold currents over a greater distance. The latter is the cause of the low temperature in the equatorial regions of the Eastern Atlantic and Eastern Pacific.

Where the sky is clear and humidity and rains deficient, very high temperatures of the air are attained, even at a great distance from the equator ( $10^{\circ}$ – $30^{\circ}$ ) and this notwithstanding winds of considerable force blowing from cooler regions. So, for example, the north winds blowing in the summer in the Sahara, and coming from the cooler Mediterranean, are certainly stronger than the trades of the ocean and yet do not prevent the desert from attaining a higher temperature than known in any equatorial region.

In the same number of NATURE you committed an error by giving the dates of freezing of the Neva in *old style*. The dates in *new style* are: Mean day of freezing, November 25, earliest October 28 (1805), latest (not quite certain), January 9 (1711), next latest December 26 (1826); mean day of opening, April 21, earliest, March 18 (1822), latest, May 12 (1810); number of days open, 218, least, 172 (1852), greatest, 279 (1822).

A. WOEIKOF

Sékpaleruaya 8, St. Petersburg, December 5–17, 1879

#### Hearing through the Mouth

THE principle of the so called "Audiphone," described in NATURE, vol. xxi. p. 243, is by no means a new discovery, although the application of it may be novel. It has long been known that sounds may be conveyed to the auditory nerves through the mouth when the drum of the ear is defective in its action, although the principle has, perhaps, been little acted upon by aurists. Mr. Rhodes's system is to press the edge of a vibrating metal disk against the upper teeth, and "the vibrations thus taken up by the disk are transmitted through the teeth and bones of the skull to the auditory nerve." (?) Such a remedy will, in many cases, be thought more inconvenient than the defect, and it is by no means necessary thus to jar the teeth and the bones. Although I am not deaf, some years ago I practised the listening to very feeble sounds through the mouth instead of by the outward ear, at the recommendation of the late Sir Charles Wheatstone. The inducing cause was to verify by experiment the true character and the notes of resultant tones, or Tartini's tones, about which no two authors had agreed. Sir Charles lent me one of his symphoniums—little instruments made like his concertinas, except that they were blown by the mouth directly upon the metal springs instead of by bellows. According to his directions I stopped my ears lightly with cotton, but pressed it into the *concha* with a thumb upon the lip of each ear. The little instrument was supported by my third and fourth fingers, leaving the notes to be touched by the first and second fingers of each hand. By thus excluding external sounds I could hear the deep and soft resultant tones to perfection; the instrument should not be tempered because they result from coincident vibrations of the notes sounded above. In these experiments I touched the symphonium as lightly as possible with elongated lips, the cavity of the mouth receiving the sounds. The teeth were covered by the lips.

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#### Intellect in Brutes

THE numbers of NATURE containing the interesting discussion on this subject have only lately reached us, and it is late to bring forward anything on the question, yet the readers of NATURE will be interested in two instances of "calculation" on the part of wild birds that I have noticed. Some years ago I was overlooking a penguin "rookery" as it is called, at the Falklands, and watching the goings on of the numerous colony below me. It was breeding season, and the birds were sitting on their eggs on